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The crime of Guiteau has directed public attention to the subject of mental diseases; we will therefore endeavor to explain the teachings of some of the most prominent of modern alienists who have recorded the results of their investigations, and classified the various phases of this, the greatest curse of humanity.

The first point of interest to be discovered is, can any line be drawn between partial and absolute insanity; if one faculty of the mind is affected do all succumb?

On this point, as on most others bearing on this subject, there is much difference of opinion; but the most advanced alienists appear to be now satisfied that a partial form of insanity exists, which is termed monomania. The German and French alienists have long since recognized this distinction and invented terms to express it, but it appears to be due to Dr. Edward C. Spitzka to have introduced this term with its proper modifications into English psychological literature.

The delusions of the monomaniac are what would be day-dreams in other people, "but which have become fixed realities for the former, owing, it is said, to a faulty cerebral association system, which permits collateral circumstances to act as supports for the patient's erroneous conception."

The general intellectual status of monomaniacs, though rarely of a very high order, is moderately fair, and generally the mental powers are sufficient to keep the delusion under check for the practical purposes of life, and although many are what is termed crotchety, irritable and depressed, yet the sole mental symptoms of the typical cases of this disease consists of the fixed delusions.

Without describing in detail the various features of monomania, let us take an imaginary case of this character, and sketch its leading characteristics. To protect us from any reproach of exaggeration or of drawing a fictitious image, we will take an extract from a paper by Dr. Edward C. Spitzka read before the New York Neurological Society, as far back as November 1880.

The monomaniac after experiences incidental to the early stages of the disease at length concludes that he is a person of some importance.

"*Some great political movement now takes place, he throws himself into it either in a fixed character that he has already constructed for himself, or with the vague idea that he is an influential personage. He seeks interviews, holds actual conversations with the big men of the day, accepts the common courtesy shown him by those in office as a tribute to his value, is rejected, however, and then judges himself to be the victim of jealousy or of rival cabals, makes intemperate and querulent complaints to higher officials, perhaps makes violent attacks upon them, and finally is incarcerated in a jail.*"

The writer of this paper had no intention of being prophetic in his utterances, but our readers cannot fail to observe the very close relation the above picture bears to any mental portrait which might be drawn of the assassin Guiteau.

It is curious that all through this train of ideas to which the monomaniac abandons himself there is seen a chain of logic and inferences; there is no gap anywhere. If the inferences of the patient were based upon correctly observed facts and associated with a proper correlation with his actual surroundings, his conclusions would be perfectly correct.

We have therefore in the monomaniac an individual with full reasoning powers, and intellectually the equal of most men. In what respect does his status differ from the sane man? The answer is, that he is possessed *with a fixed delusion* or insane project.

To follow the subject intelligently, let us now enquire what an alienist terms a delusion, and analyze its nature. This can be done profitably, for we are told that such a preliminary investigation is the most direct step for those who would be initiated into the mysteries of the insane mind.

Genuine delusions are divided into two fundamental classes; the first styled Systematized delusions as contrasted with the second class of Unsystematized delusions.

It may be here stated, that assuming Guiteau to be a monomaniac, his delusions would be of the first class.

The highest general mental development among constitutional lunatics is found among those who cherish

systematized delusions of social ambition; the delusion being often the outgrowth from a dream, or from an actual hallucination. These men usually imagine themselves the worst enemies of mankind, or are social reformers, inventors, poets, &c.; but as Spitzka remarks, it is often noticed, especially with patients of high culture, that the delusions are not so monstrous as to lead to an error in the patient's sense of identity, but limited to his self-esteem in the abstract.

Systematized delusions may also be of an expansive errotic character, when the patient constructs an ideal of the other sex, and may on some occasion discover the incorporation of his ideal, in an actual personage, usually in a more exalted position than his own. Systematized delusion may also be of an expansive religious nature, or lastly of a DEPRESSIVE CHARACTER.

We would like to give our readers a formula by which they might detect a *systematized delusion*, but although alienists are specific in their language and ample in their detail, brevity is not attempted, as perhaps not possible in treating so complicated a subject.

When, however, we see an individual without any manifest disturbance of his emotional and effective states, in full possession of the memories accumulated in the receptive sphere, and able to carry out most or all of the duties incident to his social position, yet firmly believing in the reality of that which from his education and surroundings, we should expect him to recognize as absurd, or radically wrong, the probability is that the phenomenon is due to a *systematic delusion.*

The one fundamental character which distinguishes the delusions of systematic delusional lunatics, is the correlation with their surroundings, or of their unelevative physical status. However falsely the patient's sensations and external circumstances may be interpreted, yet, after all, there is a *pseudo* logical chain running from them to the delusion which they keep to create and to sustain. This is absent in the case of patients exhibiting unsystematized delusions. Again, up to a certain stage, the *systematized delusion* is analogous to a healthy conception, this is never the case in an *unsystematized* delusion.

The factors engaged in producing the systematized delusion are two-fold. One, the predisposition we have recognized as presumably based upon anomalous condition of the brain, and the other some exciting cause which must be studied.

For instance; the general mental tone of the patient. If he be of sanguine disposition, the delusion is often the outgrowth of a day-dream, on the plan of the saying that the wish is father to the thought. If he be of a suspicious turn, delusions of persecutions are apt to arise.

Again, the physical state may influence the patient. If this be fair, delusions are apt to be expansive, and to involve social and sexual matters.

And lastly, the circumstances of the patient, as the age in which he lives, the education he receives, his social condition. All these modify the character of the delusions of this class of the insane. It is admitted that while all these factors are of the highest importance, they will never create a *systematic* delusion, unless the cerebral predisposition exists.

Of the *unsystematic delusions* we shall be brief, as they are characteristic of the acuter insanities, and are, therefore, more easily recognized. The patient exhibiting them never acts in strict accordance with his assumed character, and there is no consistency in his behavior. The unsystematized delusional lunatic will tell you that he is possessed of a million dollars, but he cannot account for his being richer to-day than he was yesterday.

It is pointed out that the great line of demarcation between the two classes of delusions lies in the fact, that, in the systematized delusion all the powers of logic and mental qualities that the man ever had are utilized by him in the construction and defense of his delusion, and as Spitzka points out is of great medico-legal importance, are also utilized in the carrying out of his schemes of defense or *revenge.* On the other hand, the unsystematized delusionist is deprived of his logical power, and apart from his hallucinations is unable to specify any support for his morbid ideas, and his actions betray that same lack of system which his delusions do.

We are indebted to two papers* by Dr. Edward C. Spitzka in presenting this classification of delusions of Monomaniacs, and we are somewhat surprised to find that he appears to ascribe all these classes of delusions to direct cerebral troubles, in fact he ridicules their being attributed to functional complications and diseases of other organs of the body.

The expert alienist can no doubt draw the distinction, and decide correctly on the true source of the mental disturbance, but it cannot be doubted that much error in this respect is exhibited by the inexperienced, for delusions of every kind are manifested at least temporally in many forms of disease, which in some cases may be so persistent as to appear chronic. As Dr. Spitzka himself frequently protests in his papers on the indiscrimate power which courts and physicians possess, who often consign "useful members of society to the living tomb of an asylum, and to the tender mercies of an ex-horse car conductor, or ex-night watchman or other politician," he will excuse the exception we take to the too ready desire of many persons to place in the category of "maniacs," men who are merely hypochrondriacal or depressed and vicious in disposition.

We have indicated the form of insanity, which may be ascribed to the case of Guiteau, but we have no desire to prejudge the case. The crime was barely committed before Cabinet Ministers, Physicians, Editors, and a large portion of the public, immediately jumped to the conclusion that the assassin was mad; that such a verdict was hastily given all must now admit. Whether the evidence, which will undoubtedly be produced at the trial, will justify the first impression, and release the prisoner from the responsibility of this crime, will be a matter to be watched with considerable interest.

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*Insane delusions, their mechanics and their dragnostic bearing. The Journal of Nervous and Mental Disease, January, 1881. Monomaniac or "Primære Verrueckthcit." Read before the Neurological Society, Nov. 5, 1880. Reported in St. Louis Clinical Record.

We recently called attention to a report made by Professor Leeds to the Chemical Society, of New York on the adulterations of certain articles of food. The tenor of the report was to show that food products in general were unadulterated and pure, and to cast ridicule on those who asserted to the contrary. Among other specific statements Prof. Leeds stated that he had made a special examination of sugar syrups, and asserted that the result of his investigations showed, that they were free from any admixture of glucose.

Side by side with Prof. Leeds' report we gave the statement of Prof. Wiley that 500 tons of glucose was made daily in the United States, the bulk of which was used for adulterating cane sugars, and that the glucose of commerce as sold in the Western States was largely composed of syrup made from starch.

We publish in this issue a letter from Prof. Wiley in confirmation of his report, stating that the manufacture of a sugar, which is a mixture of glucose and cane sugar, is carried on in New York city or its vicinity.

AMYLOSE.

As a thousand tons of sugar made from starch will within a few months be placed on the market daily, half that amount being already the consumption of that article of commerce, it appears desirable to make use of some name by which this substance may be known and at the suggestion of Prof. Wiley, we propose "AMYLOSE" as an appropriate term.

AMYLOSE will include all varieties of syrups and sugars manufactured from starch. (Lat. *Amylum, Starch*).

NOTE ON PHOTOGRAPHS OF THE SPECTRUM OF THE COMET OF JUNE, 1881.

By Professor Henry Draper, M. D.

The appearance of a large comet has afforded an opportunity of adding to our knowledge of these bodies by applying to it a new means of research. Owing to the recent progress in photography, it was to be hoped that photographs of the comet and even of its spectrum might be obtained and peculiarities invisible to the eye detected. For such experiments my observatory was prepared, because for many years its resources had been directed to the more delicate branches of celestial photography and spectroscopy, such as photography of stellar spectra and of the nebulæ. More than a hundred photographs of spectra of stars have been taken, and in the nebula of Orion details equal in faintness to stars of the 14.7 magnitude have been photographed.

It was obvious that if the comet could be photographed by less than an hour's exposure, there would be a chance of obtaining a photograph of the spectrum of the coma, especially as it was probable that its ultra-violet region consisted of but few lines. In examining my photographs of the spectrum of the voltaic arc, a strong band or group of lines was found above H, and on the hypothesis that the incandescent vapor of a carbon compound exists in comets this band might be photographed in their spectrum.

Accordingly, at the first attempt, a photograph of the nucleus and part of the envelopes was obtained in seventeen minutes on the night of June 24th, through breaks in the clouds. On succeeding occasions, when an exposure of 162 minutes was given, the tail impressed itself to an extent of nearly ten degrees in length.

I next tried by interposing a direct vision prism between the sensitive plate and object glass to secure a photograph which would show the continuous spectrum of the nucleus and the banded spectrum of the coma. After an exposure of eighty-three minutes, a strong picture of the spectrum of the nucleus, coma and part of the tail was obtained, but the banded spectrum was overpowered by the continuous spectrum.

I then applied the two-prism spectroscope used for stellar spectrum photography, anticipating that although the diminution of light would be serious after passing through the slit, two prisms and two object glasses, yet the advantage of being able to have a juxtaposed comparison spectrum would make the attempt desirable, and moreover, the continuous spectrum being more weakened than the banded by the increased dispersion the latter would become more distinct.

Three photographs of the comet's spectrum have been taken with this arrangement with exposures of 180 minutes, 196 minutes and 228 minutes, and with a comparison spectrum on each. The continuous spectrum of the nucleus was plainly seen while the photography was in progress. It will take some time to reduce and discuss these photographs and prepare the auxiliary photographs which will be necessary for their interpretation. For the present it will suffice to say that the most striking feature is a heavy hand above H which is divisible into lines and in addition two faint bands, one between G and *h* and another between *h* and H. I was very careful to stop these exposures before dawn, fearing that the spectrum of daylight might become superposed on the cometary spectrum.

It would seem that these photographs strengthen the hypothesis of the presence of carbon in comets, but a series of comparisons will be necessary, and it is not improbable that a part of the spectrum may be due to other elements.

271 Madison Avenue, New York.

OBSERVATIONS ON SIREDON LICHENOIDES.*

By Wm. E. Carlin.

Como Lake is a body of water about two miles and a half in circumference. It has no known outlet, but is fed by a stream of pure spring water about two feet wide and a foot deep, which, continually running, prevents the lake's absorption by evaporation. The lake is quite shallow and can be easily waded at almost any part, being not more than 10 feet deep in the deepest place that I have been able to find. The bottom of the lake is soft and is covered in most places with grass and weeds. The water is strongly impregnated with alkali, and a large number of cattle are said to have died a number of years ago from drinking it. It is very disagreeable to the taste. The amount of water varies about 14 inches during the year, being highest in the spring from the melting snows, and lowest in the autumn. This is the home of the *Siredon lichenoides* (Baird). They never enter the stream of fresh water, preferring the alkali water of the lake. They seem to suffer no inconvenience, however, if placed in fresh water. I have caught as many as a hundred and fifty and

*From the Proceedings of United States National Museum.

placed them in a cauf, and have never had one die from the change. The change to fresh water undoubtedly hastens the metamorphosis into the *Amblystoma* form, as I have noticed quite a change in the course of twenty-four hours in individuals placed in the cauf, while an equal number kept in the alkali water in the boat have shown no change in any of them in several days. I have kept six at different times in jars of fresh water until they have completed their metamorphosis. I made no systematic note of appearance from day to day, but my observation was careful and regular. In two cases the change in external appearance was so abrupt that I would have been almost certain that another salamander had been substituted for the one in the jar had I not had him so completely under observation that it was impossible. The gills had assumed a stubby form about half the length that they were the night before, and the gill on the back on the back of the body was nearly half gone; it took air quite often, and I removed it from the jar and placed it in a box with some lake grass around it to keep it moist. It completed the metatmorphosis in a few days. I did not feed it any during this time. While it was in the jar it was well fed with flies. The jar was placed upon a table in the telegraph office. The flies at first had to be pushed in front of it with a pencil. It finally got to know that tapping the jar with a pencil meant a fly, and would rise to the surface immediately and snap at whichever it saw first, pencil or fly. It furnished train-men continual amusement while here, and they kept it constantly gorged. Those that I kept well fed in jars and seldom changed the water, say once in three days, usually began to show a slight change in from two to three weeks, and all of them completed the change into the *Amblystoma* inside of six weeks, while I have had but three changes of those kept in the cauf (sixty of them) in three months. During that time they have not been fed at all. The *Siredon mexicanus* is said to never undergo the transformation in its home, and Professor Marsh doubts that it ever makes it here. This doubt I can put at rest. They do make the change here, and in large numbers. During the latter part of the month of July and the entire month of August, if the day is rainy or misty, they come from the lake to the shore in large numbers, and secrete themselves under some piece of wood or rock where they can keep moist. Sometimes they venture out in a shower, and the sun catches them before they can obtain shelter either in the lake or under cover, and in a few minutes kills them. They can be found dried hard anywhere about the lake, on the shore or in the grass. While catching *Siredon* I have seen and caught a number of *Amblystoma* in the lake, with the metamorphosis, as far as I could see, as complete as those we find half a mile from the lake. They cover the ground by thousands during a warm summer rain, coming from every conceivable place where they could have found shelter, from under rocks, boards, old ties, and out of gopher holes. I have a cat that eats them greedily. She has fished several out of jars on the table and devoured them during the night when there was no one to watch her; and I am told by a resident that the numerous skunks that live around the lake live principally on them. They are of two colors, a blackish green and a yellowish green color. I have had two of the blackish green complete the change in sequence, while one of the yellowish green was completing it under the same circumstances of change of water and food. I think this will be found to be the result in all similar cases. I have caught them in all stages of growth and in all stages of their changes into the *Amblystoma* state. During the months of July and August they lie close to the shore of the lake, where it is shallow; but after the first frost they disappear completely, or at least I have never been able to find them. I think they must bury themselves in the mud at the bottom of the lake, as I have stirred up the grass often and have not seen them issue from it.

AN ANALYSIS OF WATER DESTRUCTIVE TO FISH IN THE GULF OF MEXICO.*

BY F. M. ENDLICH.

Having completed the examination of sea-waters from the Gulf of Mexico, so far as the scant supply would permit, I have the honor to offer the following report thereupon, the water in which the fish die being designated as A, the good water as B:

	A.	B.
Specific gravity....................	1.024	1.022
Solid constituents (total), per cent...	4.0780	4.1095
Ferric compounds, per cent.........	0.1106	0.0724
Injurious organic matter...........	ratio=3	ratio=2

I find that the water A contains a large quantity of *Algæ* and *infusoria*. It is eminently probable that the former may have had an injurious effect upon the fish. Specimens of the algæ have been submitted to Professor Goode, who will send them to some expert, in order that their specific gravity may be determined.

The "dead fish" in possession of the United States National Museum are such that any examination of the organs of respiration will be of no avail.

I cannot find, even by spectroscopic analysis, any mineral constituents in the water A which could noxiously affect the fish.

In my estimation the death of fish was caused by the more or less parasitic algæ, which are found in large quantities in water A, but do not occur at all in water B.

In case the same phenomenon should recur, the presence of an expert in the questions involved, more particularly chemistry and botany, would most likely lead to definite results,

Prof. S. F. BAIRD,
Secretary the Smithsonian Institute,
WASHINGTON, D. C.

A MICROSCOPICAL STUDY OF THE IRON ORE, OR PERIDOTITE OF IRON MINE HILL, CUMBERLAND, RHODE ISLAND.†

BY M. E. WADSWORTH.

The attention of the writer was first particularly called to this formation by some specimens presented to him by Mr. H. B. Metcalf in the Spring of 1880. These did not appear to the writer to be any common ore of iron, but rather fragments of a basic eruptive rock containing much iron. Sections were accordingly made which revealed its true character.

The formation was described by Dr. Charles T. Jackson in his report on the Geological Survey of Rhode Island in 1840. He states that Iron Mine Hill "is a mountain mass of porphyritic magnetic iron ore, 462 feet in length, 132 feet in width, and 104 feet in height above the adjoining meadow. From these measurements, which were made over only the visible portion of this enormous mass of iron ore, it will appear that there are 6,342,336 cubic feet of the ore above natural drainage. ...Its specific gravity is from 3.82 to 3.88....This ore is remarkable both on account of its geological situation and its mineralogical and chemical composition. It appears to have been protruded through the granite and gneiss at the same epoch with the elevation of numerous serpentine veins which occur in this vicinity. This will appear the more probable origin of this mass, when we consider its chemical composition in comparison with that of the iron ore, which we know to have been thrown up with the serpentine, occurring on the estate of Mr. Whipple, and the fact that the ore at Iron Mine Hill is accompanied by serpentine mixed with its mass in every

* From the Proceedings of United States National Museum.

† From the *Bulletin of The Museum Comparative of Zoology*.—Harvard College.

part, gives still greater reason for this belief." (*l. c.*, pp. 52, 53.)

He gives as the result of his chemical analysis of the "Porphyritic Iron Ore from Iron Mine Hill, Cumberland," the following (*l. c.*, p. 53):—

SiO_2	23.00
Al_2O_3	13.10
Fe_2O_3	27.60
FeO	12.40
MnO	2.00
MgO	4.00
TiO_2	15.30
H_2O and loss	2.60
Total	100.00

In 1869 the Rhode Island Society for the Encouragement of Domestic Industry published a report relating to the coal and iron in Rhode Island, from which we glean the following. The iron ore is regarded as practically inexhaustible, the mass at Iron Mine Hill visible above drainage being estimated at two millions of tons.

"It is also conceded, as regards quality, that the Cumberland ore is free from sulphur and phosphorus, the most common and worst impurities, and that it contains manganese, the most prized of all the elements found in connection with iron. For these reasons the Cumberland ore is sought by manufacturers at a distance, to mix with softer ores and improve their quality, and is now exported from this State for that purpose."

It seems that this Iron Mine Hill ore was employed in 1703, mixed with the hemitite of Cranston, R. I., for the casting of cannon. The work was done at Cumberland, and, in part at least, "the cannon used in the celebrated Louisburg expedition, in 1745," were cast from these ores. The manufacture was abandoned in 1763, owing to an explosion of the furnace, by which the proprietor was killed.

During the administration of John Adams the same ores were also used for the manufacture of cannon. It seems that the Cumberland (Iron Mine Hill) ore was employed in the manufacture of charcoal iron at Easton, Chelmsford, and Walpole, Mass., as late as 1834. "The Cumberland ore, mixed with equal quantities of Cranston hematite or bog ore, produced, for a long period, a charcoal iron unsurpassed in this country. . . . The Cumberland ore contains an uncertain percentage of titanium, which, while it improves its quality, helps make it refractory. The ore is porphyritic, the magnetic oxide being associated with earthy minerals, principally feldspar and serpentine." It would seem that in 1869, and before, the ore was largely shipped to Pennsylvania to mix with other ores.

A letter of Professor R. H. Thurston, published in this report, states: "The Cumberland iron ore is of the kind known to mineralogists as 'ilmenite;' among mettalurgists as 'titaniferous magnetic ore,' and iron manufacturers, on account of its peculiar value for producing steel, would term it a 'steel ore.' . . . The Cumberland ore is conveniently located and of inexhaustible extent; it is perfectly free from noxious elements, though somewhat refractory; it will furnish a very strong iron or a most excellent steel; it can be smelted within the State at a profit; it can be made directly into steel at a much greater profit; steel made from it will bring the highest prices in the market."

Professor Thurston states that the mean of various analyses made of this ore is about as follows:

SiO_2	22.87
Al_2O_3	10.64
Fe^2O^3 } FeO }	44.88
MnO	2.05
CaO	0.65
MgO	5.67
TiO	9.99
Zn	0.20
H_2O and loss	3.05
Total	100.00

The ore on one side of the hill, where it has been most extensively quarried, shows a dark, somewhat resinous groundmass, holding large striated crystals of feldspar. The resinous lustre and greenish-yellow color, as observed under the lens, are caused by the presence of olivine. The olivine becomes more strongly marked on the slightly weathered surfaces seen on the faces of the quarry. Under a lens of high power, the olivine shows clearly on the fresh fractures. The olivine in weathering decomposes to a yellowish and reddish-brown ferruginous powder, leaving the other constituent of the rock, the magnetite, well marked. The magnetite decomposes more slowly, and forms an incoherent mass after the decay of the olivine. The rock gelatinizes with hydrochloric acid, and yields a titanium reaction. A fragment allowed to stand a day or two in weak hydrochloric acid yielded gelatinous silica copiously.

A section made with special reference to the feldspar crystals shows large porphyritic crystals of the latter enclosed in a mass of magnetite and olivine.

The magnetite forms irregular, more or less connected masses, making a sort of sponge-like structure. Its rounded and irregular cavities are filled with olivine, which also occupies the interspaces between the magnetite masses. The olivine is in rounded forms, which sometimes show one or more crystal planes. It is cut through by numerous fissures, that usually show a ferruginous staining along their sides. The olivine also holds grains of the magnetite. Except the fissuring and ferruginous staining, the olivine is comparatively clear, and shows little signs of alteration.

The plagioclase feldspar shows well-marked lines of cleavage and fracture, and is somewhat kaolinized along these lines. It contains a few irregular flakes of biotite together with grains of olivine and magnetite.

The order of crystallization appears to have been, first the magnetite, then the olivine, and lastly the feldspar.

This rock is similar to the celebrated iron ore of Taberg, Sweden, as described by A. Sjören in the Geologiska Föreningens Forhandlingar (1876, III. 42-62; see also Neues Jahrbuch für Mineralogie, 1876, 434, 435.) The Taberg rock has been worked as an iron ore for over three hundred years. This Swedish ore is called by Sjören "magnetite-olivinite."

The feldspar is confined to the peridotite found on one side of the hill, where the peridotite passes into a compact greenish-black rock, showing patches of serpentine and grains of magnetite. From this fact it seems necessary to regard the feldspar as abnormal and local in the rock, which in general is composed of olivine and magnetite or their alterative products.

The structure remains about the same in the non-feldspathic portions as it is in those before mentioned as holding feldspar. But the olivine is entirely changed to a greenish serpentine which shows beautiful fibrous polarization. The serpentine retains the form of the olivine grains, their inclusions, and the network of fissures before mentioned. In some of the sections considerable carbonate was seen, presumably dolomite. In one section part of the olivine grains, especially towards their interior, remained unchanged, but on their edges they were altered to serpentine. Another change was observed here: the formation of secondary crystals of irregular outline that belong probably to actinolite. Some are elongated and narrow; other are short and broad, traversed, by cleavage planes. They evidently belong to the monoclinic system.

The origin of this rock could not be told from its field relations, as its contact with any other rock could

not be found. Since the only method in which its origin can be absolutely shown cannot be used without expensive excavation, it only remains to give the probabilities so far as ascertainable from the mass itself. Such microscopic characters and mineral association have been, so far as we know, only found in eruptive rocks when the origin of such rocks has been studied with sufficient care to determine it. Hence we must conclude it is most probable that this mass is eruptive also, until found to be otherwise.

It closely resembles in structure and composition some of the meteorites, except that its iron is oxidized and not in a native state—a resemblance which for others of the peridotites has long been pointed out. It is rocks of this character, as has been suggested by others, that give us the most probable clew to the interior composition and structure of the earth.

The rock in the field shows, to our mind, no signs of structural planes that should be referred to sedimentation. On one side the rock is massive and jointed, and on the other it is jointed in fine parallel planes. This portion of the rock is more highly metamorphosed than the other, and, as is usual in highly altered eruptive rocks, joints parallel to certain lines of pressure occur. The writer has seen this structure in many rocks that were indisputably eruptive, forming well marked dikes in other rocks.

A rod away from the main mass of the iron ore, near one end, some serpentine appears that cannot be directly connected with the other peridotite. Microscopically its characters and structure are the same as the main rock, and there is no reason to regard it as distinct. The rock nearest to the peridotite is a mica schist some hundred feet away. It shows no characters that would indicate the transition of the ore into it.

The locality was visited by the writer in October last, in company with Professor A. S. Packard, Jr., of Brown University, and Mr. T. S. Battey, of the Friends' School, Providence, R. I. To the latter gentleman I am especially indebted for a copy of the paper of the Rhode Island Society before mentioned, and for other favors.

This examination may serve as an illustration of the aid that microscopical lithology may be to the practical side of life, since now, for the first time since this rock has been worked, can the ironmaster who wishes to use it approach understandingly the metallurgical problems it presents; whether he desires to employ the rock as a whole, or to concentrate the magnetite first.

In direct-vision spectroscopes the number of prisms involves a considerable loss of light. M. Zenger now uses a liquid prism of ordinary form, having attached on its anterior plane a quartz prism of the same refringent angle, but arranged in opposite direction. The posterior face of the liquid prism carries a plane parallel plate. The rays fall normally on the quartz. The loss of light is by this arrangement reduced to a minimum. The spectra obtained are very intense, and the lines are well defined. A single parallelepiped of the kind decomposes the D line to the naked eye, and with a small Galilean telescope, magnifying five times, one can distinguish the difference of breadth of the two lines, and easily see the extreme red and ultra-violet rays, though there are only two prisms of 60 degrees.

M. Poliakoff, the distinguished Russian naturalist, has examined a horse presented by Colonel Prejvalsky to the St. Petersburg Academy, and decides it to be a new species, which he has named *Equus Przewalskii*. A translation of his memoir appears in the "Annals of Natural History," and from this it appears that the new representative of the family of undivided-hoofed mammals is in some respects intermediate between our domestic horse and the wild ass, but it differs from the asinine genus in having four callosities, one on each leg. In the form of skull, absence of dorsal stripe, and other particulars it resembles the domestic horse. This newly-recorded animal is indigenous to the plains and deserts of Central Asia, and has not hitherto fallen under the dominion of man.

COMET (*b*), 1881.

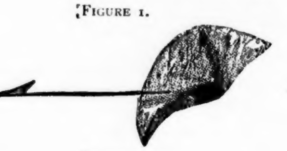
We continue the interesting series of sketches of this comet, made by Professor Edward S. Holden with the 15-inch equatorial at the Washburn Observatory, Madison, Wisconsin.

July 13, 1881.

9h. 30m.

Figure 1.

July 14, 1881.



10h. 20m.

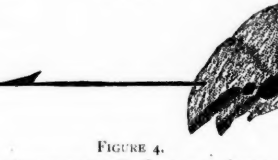
Figure 2.
Moonlight.

July 17, 1881.

10h. 45m.

Figure 3.

July 18, 1881.



9h. 30m.
11h. 0m.

Figure 4.

The nucleus is DOUBLE (it has not been previously) $p = 275^\circ \pm$, $s = 1''.5$, with a dark space between the parts.

DO WE SEE NON-LUMINOUS BODIES BY REFLECTED LIGHT?

By A. G. Gaines, Pres. St. Lawrence University, Canton, N. Y.

All who have treated this subject have answered the above question with an unequivocal *yes*.

It may appear presumptuous to call the answer in question. Nevertheless, while reflecting recently on some of the peculiar facts of light and vision the thought came to me to doubt this universally accepted proposition; and now I wish to express my more confirmed doubts, and give some reasons for thinking we must revise our views on this point to some extent.

What I now hold is that neither *transmitted* nor *reflected* light reveal to us in vision either the body transmitting or the body reflecting, but that *radiant* light does reveal in vision the radiant body, and that the light by which any non-luminous body is visible is essentially of the nature of radiant light, and is properly to be so called. Paradoxical as these views may seem on bare statement, I think that a little consideration of the facts involved will soon convince us that they must be accepted as true, and show us that the present paradox is due to the illusions of an erroneous point of view.

It is a known and universally accepted truth that *transmitted* light does not reveal the transmitting medium. It may be refracted, little or much, but when it reaches the eye it reveals, not the refracting medium, but the body from which it was emitted. The refracting or transmitting body may be *visible*, but is not visible by transmitted light. Were it perfectly transparent, that is, were it to transmit *all* the light coming to it, it would be invisible. This is no new truth, but one universally held and taught; and thus far we are all agreed.

Now let us attend to *reflected* light. As we attend to it we shall learn that reflected light does not reveal the reflector but the body emitting it. If bodies are seen by reflected light, they should be more clearly seen in proportion as they reflect more perfectly the light falling on them. The facts are exactly contradictory to this. In proportion as any given surface is a good reflector it is to that degree invisible, and when a surface becomes a perfect reflector it becomes invisible. Can it then be true that bodies are seen by *reflected* light when it is palpably true that the better they reflect light the less visible they are? The reflected light makes visible the body emitting it, not the reflecting body, and it results that, in studying the stars, the astronomer uses nearly indifferently a reflecting or a refracting telescope. Plainly then, we would say, it is not by reflected light that bodies are visible. This conclusion cannot be escaped by any conjectures as to the extent and form of the reflecting surface. The minutest surface reflecting the sunlight gives a brilliant, dazzling star, not a revelation of itself. Curved, convex or concave, or variously warped surfaces give only images variously enlarged or diminished, or variously distorted, of the body emitting the light, and not at all of the surfaces reflecting it. If the microscope be applied to the surface, the facts are still found to be as above stated. No theory of minute reflecting surfaces changes any of these facts, unless it were imagined that a surface might be so small as to *decompose* the light falling on it, but this result would be destructive of the theory now objected against. Thus it appears from all the facts stated and referred to that the proof is conclusive that, in no case is a body seen as such by the light it *reflects*.

If, now, we go on to inquire as to the light by which bodies are seen, we may find some good reasons for believing it to be essentially *radiant light*, even when proceeding from non-luminous bodies. Note, then, that it is the peculiarity of radiant light that it is emitted in straight lines in every possible direction from every luminous point. The light, hence, by which such a point or body is seen is *divergent* light, and the office of the optical apparatus is to bring it to a focus on the retina. It is not possible for a single point (the minimum of visible surface,) in any reflecting surface to reflect light in every direction; and for light thus to proceed in every direction from a luminous point is the distinguishing characteristic of radiant light. What thus characterizes the light of what are called luminous bodies will be found to characterize the light by which all non-luminous bodies are visible. From every point of any such visible body the light proceeds in every possible direction; whence we note that every such point is a point of dispersion or radiation, and not a point of reflection. Here, as we learned in the case of luminous bodies, the light by which any ordinary non-luminous body (so-called) is seen is *divergent*, and the office of the optical apparatus is to bring it to a focus on the retina. This brings before us the perfect similarity of the conditions under which luminous and non-luminous bodies are seen; and which seem to compel us, hence, to regard the light by which non-luminous bodies are seen as having essentially the same qualities and relations as radiant light.

If, now, we seek to know how this can be explained, seeing that non-luminous bodies are not original sources of light, I think we may find a nearly perfect analogy in the facts of heat that may afford us much help. We are tolerably familiar with radiant and reflected heat. The heat which a body reflects follows all the laws of reflected light, and has this peculiarity, that it does not change the temperature of the reflecting surface. For the rest, the heat which falls on a body, and, as it is said, is *absorbed* by it, raises the temperature of the absorbing body, and immediately said body begins to *radiate* heat, and the heat thus radiated shows all the essential characteristics of radiant heat. What we wish to have particularly noted here is, that this *heat* has been all along said to be *radiated, not reflected*. By the principle of the correlation of forces the heat which is said to be absorbed is transformed first into increased molecular activity in the absorbing body, and then again transformed into what is emitted as radiant heat; and this emission is in straight lines in every direction from every point in the surface of the body radiating. All this is plain, and in perfect agreement with the accepted theory of heat. We have now only to apply these facts and principles, by analogy, to light, and we may obtain an equally plain and consistent theory of light as to visible bodies.

We have already called attention to the fact that the light which a surface *reflects* does not reveal that surface. The light by which any non-luminous body is seen is emitted, let us say, *radiated*, from every point of its surface. This may now be explained by supposing the light (luminous energy) received by such a body as in some degree or manner absorbed by the superficial particles of the body, and then radiated from every such particle as a centre, analogous to what we believe of heat. The light thus taken in appears to be always *decomposed*, with numberless variations of results; so that the light emitted or radiated is always of a different *color* from that received. This difference of color affords us another contrast between the light by which bodies are seen and reflected light; this last being always of the same color as the incident light. In making this statement we have in mind the fact that the same surface may both reflect and radiate light; and that, hence, in each case we must take care not to confound the one with the other in making our observations. When this caution is observed, the statement above concerning the color of reflected light will not, we think, be called in question.

The explanation, then, that I would offer is, that the light which falls on non-luminous bodies (so far as it is not reflected) is somehow absorbed by them, decomposed, and then radiated, at least in part, that the body is visible by this *radiated* light, and not at all by that light which it reflects. In these actions and reactions between the luminous energy falling on a non-luminous body and the body itself, we think it not improbable that there are some correlations of force; and that these may be essential parts of the change that enables the light radiated to make visible the non-luminous body.

If the views presented in this paper be allowed, they enable us to place the facts of phosphorescence, and may be of fluorescence, in harmony with the action on light of ordinary non-luminous bodies; and differing from these chiefly, if not wholly, in *degree* only. And is it not true that this so-called phosphorescence is possessed in some degree by every visible body? We do not now speak of cases of slow combustion, like exposed phosphorus, but those continuing to emit light for a time after being cut off from extraneous light, like snow and the diamond. We would look for the explanation of these greater degrees in phosphorescence in the power of the bodies exhibiting it to absorb and decompose light more deeply, and then more tardily radiate the luminous energy, than is true of non-luminous bodies generally.

It may be proper here to notice the facts of *iridescence*, with which our theme may have some interesting connections. Inasmuch as the facts of iridescence are explained by the interference of the luminous waves, caused by the reflection of light from very thin laminæ, it might be thought the same explanation would apply to *decomposition* of light by ordinary non-luminous bodies. We think the facts in the two cases so different that the same explanation is not applicable to both. In the first place, the facts of iridescence agree with the usual characteristics of *reflected* light; while, on the contrary, we have noted in this paper that the facts in the case of ordinary visible bodies do not so agree. And, in the second place, the results of the decomposition of light in iridescence

agree with the results obtained by prismatic decomposition; while the results in the other case do not. We think it would be correct to say that iridescence does not reveal non-luminous bodies in the same way, nor with the same certitude, as that light reveals them by which they are ordinarily visible. In making this last statement we have in mind the fact that the iridescent surface, in addition to its iridescence, also emits or radiates light in the same manner as ordinary visible bodies; and that these two facts are not to be confounded in our observations and reasonings. Without pursuing the subject further into details, these are some of the reasons why we think the facts of iridescence are not inconsistent with the main doctrine of this paper.

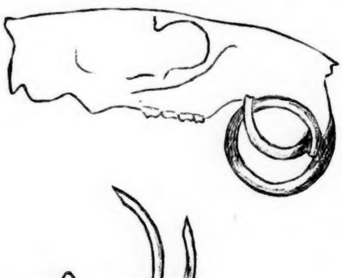
We conclude then, by reason of the facts and relations to which we have now called attention, we cannot believe that it is correct to say that non-luminous bodies are seen by *reflected light*; and we offer the suggestion that the light by which such bodies are seen should fairly and properly be called *radiant light*, as manifesting all the essential qualities of such light.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE."

In an article on overgrown teeth of Fiber Zibethicus (which by a singular typographical error is printed Fiber Wibethicus) in "SCIENCE" for July 16th, the writer describes a not very uncommon phenomenon among rodents to which I can add an interesting example.



The inclosed drawing represents a similar case, being a woodchuck (Arctomys monax); it will be noticed that one of the upper teeth has grown far enough to form a semicircle while the other upper incisor has described a somewhat larger curve and finally thrust itself through the first and then continued to form a *complete circle*, as will be evident from the figure. This specimen was mounted here (with one other similar but not so extreme a case) and is now in the Museum of Comparative Zoology at Cambridge. F. W. STAEBNER.

July 20, 1881.
WARD'S NATURAL SCIENCE ESTABLISHMENT, Rochester, N. Y.

COMET (c) 1881.

To the Editor of "SCIENCE."

The comet discovered by Mr. Schaeberle at Ann Arbor, July 13, promises to become a very interesting object, not only because it will soon be visible to the naked eye, but also because its orbit shows great similarity to the great comet of 1337, as may be seen by the following comparison:

	1881 (Stone)	1337 (Hind)	1337 (Lanzier)
Distance of perihelion from node..	122° 30′	108° 44′	90° 41′
Longitude of node................	98 43	99 6	93 1
Inclination......................	141 35	137 6	139 32
Logarithm perihelion distance....	9.7959	9.97	9.92

The difference between the orbits of the two comets is perhaps not greater than the uncertainty of that of 1337. The latter was first seen in China on the 26th of June, and afterwards in Europe on the 24th of October.

Schaeberle's comet has been observed here on a number of mornings, and its increase in brightness has been quite perceptible. This morning the tail was very apparent, the sky was very cloudy, or I presume it would have been visible to the naked eye. It ought to be quite plainly visible at any rate before the end of this week. It will be at perihelion and nearest the earth about the 20th of August, and will remain at approximately the same distance from us for a week or more. A few days before that time its right ascension will have become equal to the sun, so that when at its greatest brilliancy it will be visible in the evening. While it will undoubtedly become a magnificent object, it will not probably equal the great comet now receding from us.

ORMOND STONE.

MT. LOOKOUT, O., *July* 25, 1881.

ASTRONOMICAL NOTE.

WASHBURN OBSERVATORY, UNIVERSITY OF WISCONSIN, MADISON, WIS., July 17, 1881.

To the Editor of "SCIENCE."

Among the new red stars found here, the following is by far the finest and may be of interest:
Anon. 9 mag. R. A. $1^h\ 48^m\ 45^s$: Dec. = + 58° 40′.2 1880.0. EDWARD S. HOLDEN.

ADULTERATION OF SUGAR.

To the Editor of "SCIENCE."

DEAR SIR—In the leading editorial of "SCIENCE" of June 18, you speak of the different results obtained by Prof. Leeds and myself of examination of commercial sugars and syrups for glucose and grape sugar. I can only take exception to one statement contained therein, *i. e.*, the one which intimates that these different results form the theme of a scientific controversy. Since the reception of your letter I have renewed my inquiries for statistics, and can now say that I do not believe my estimates of the quantities made in the United States are very wide of the truth. Dealers and manufacturers are extremely reticent on the whole subject, and it is only by hard work and often indirection, that one can get at the truth. In your own city, New York, there is a large establishment for making "New Process Sugar," the Manhattan Refining Company, unless it has lately changed its name. Yet a prominent New York chemist stated publicly, and published over his own signature, that he had made diligent search for this establishment, and it could not be found. At the same time, to my personal knowledge, a western firm had just received a large consignment of "New Process Sugar" from this firm.

At the Boston meeting of the A. A. A. S., I stated on the strength of this personal knowledge that I believed the Manhattan Company was no myth. This statement was published in the Boston and New York papers, and was seen by the proprietors of the Manhattan Company. They wrote to assure me that I was right in my statement, sending me at the same time samples of their different sugar for analysis.

Within the past year the mixing of sugars has largely increased, and is now carried on in New York, in Buffalo, in Chicago, and at other points. A prominent

sugar dealer has just told me "some of these establishments turn out five hundred barrels daily."

From the best information I can get, I would place the daily yield of mixed sugars at 1500 to 2000 barrels. This, remember, is only a careful estimate from all the data I can get. The process is increasing just as fast as *dry white* grape sugar can be made.

With regard to table syrup, I can only reiterate my former statements. In response to a recent inquiry a prominent dealer has just written me as follows:

"My observation leads me to believe that fully four-fifths of all the syrup sold throughout the western and northwestern States and Territories are composed of glucose, with enough cane syrup or molasses added to give it the color most desirable to the buyer or consumer."

I have no accurate information respecting the eastern trade. I am not without a suspicion, however, from the appearance of some syrups which I have lately seen on Boston tables, that the beautiful syrup made from the corn of our western prairies, has invaded the very stronghold of the dirty refuse syrups of the sugar refineries.

Respectfully,

H. W. WILEY.

BOSTON, *July* 27, 1881.

WATER AS FUEL.

To the Editor of "SCIENCE":

I am interested in the paper of Dr. Rachel, in "SCIENCE," July 9, on the use of water in combustion.

The subject is a most important one; and I regret a mistake which a reader might fall into from an inadvertency, I suppose, in not more clearly distinguishing between the degrees of temperature at which the transfer of oxygen takes place from the hydrogen of the water to the carbon set free by the dissociation of the naphtha, and the number of heat units set free or absorbed by such transfer, which is a very different thing.

It is not a case of the dissociation of water, but of the dissociation of naphtha, and the *transfer* of oxygen from the hydrogen to the carbon so set free; a carbon which was very loosely held by the hydrogen in naphtha.

It is only a trade of so much carbon for so much hydrogen; the absolute heat of which, according to authorities, are almost exactly equal in complete burning.

But while the trade is thus equal in absolute heat, there is practically an enormous gain; and it is very important to see just what it is.

We get our heat all in hydrogen instead of in carbon, and so avoid the enormous practical losses which attend the usual mode of burning carbon.

We get our heat in hydrogen, which is the easiest of all things to get all of it out by burning, instead of in carbon which is one of the hardest of all things to get it all out of, the difference being much like that between the ease of getting our money out of a bank or out of a lot of debts where from half to three-fourths or nine-tenths is almost always finally lost.

The practical man does not understand very well what it means when he is told that the use of coal under boilers only produces five per cent of its energy in work. It means this: that a series of enormous losses is made in trying to get the heat out of the carbon in burning. An enormous quantity goes off as black smoke and soot, not burned. An enormous quantity goes off as carbonic oxide, giving up only a third of its heat, instead of giving it all up by burning to carbonic acid. An enormous quantity of heat is lost in heating up the great quantity of gases and air, that pass off without being fully burned, which prevents a high temperature. An enormous quantity of heat is carried up the chimney, because a little coat of soot and ashes on the boiler does not allow it to go into the boiler fast enough while passing so rapidly to the chimney. Is it any wonder that an enormous revolution is to be made by a mode of burning where water is used not for any energy to be got out of it, but by which the heat is taken from carbon, and put into hydrogen, where it all flashes into usable form the instant the air can reach it? Where the whole heat is liberated at one point where it is wanted, instead of in a long, imperfect flame? Where there is not a particle of soot or dust to tarnish the boiler and prevent the heat from passing into it wherever it strikes? And where the heat is nearly all in that low form of invisible radiance which is best suited to radiate on to, and be absorbed by, the boiler without waiting for the heated gases to actually strike it to give up their heat by slow convection?

It is a fact well known in the use of the alcohol and gas flames, that if you want them for heating alone you want the non-luminous flame only; because with a luminous flame a large part of the heat is in the form of light, which is not so readily absorbed by substances as heat, as it is with the non-luminous flame; and another large part is held by the gases as convective heat, and cannot so freely pass into substances by radiation, as in the radiation from the non-luminous flame.

Thus the revolution in combustion by the use of water consists in transferring the heat from carbon to hydrogen, by which all the great losses of burning are avoided, and the entire heat is obtained clean, without smoke, at the exact point desired, and in the form which takes right hold of the work without loss of time or energy.

These things are of an incalculable value; very much better than any mysterious supposed increase of heat from the water itself.

Though it is important to form a non-luminous gas by means of water to utilize the carbon, yet the water so added takes up a share of the heat to raise its temperature along with the other gases, and so reduces the temperature in proportion to its quantity.

So, if we need high heats we must use the least quantity of water, which will turn all the carbon into a suitable gas.

If we add water enough to make the carbon of 100 pounds of naphtha into carbonic acid, it will take 250 pounds of water, if we assume that the naphtha averages a composition of $C_6 H_{14}$, containing 84 pounds of carbon and 16 pounds of hydrogen; and the result would be 30 pounds of hydrogen and 310 pounds of carbonic acid. This would require 1530 pounds of air to burn it, and produce 1880 pounds of gas, of which over one-seventh would be due to the water, and the temperature would be less than $\frac{4}{7}$ of what it would have been without, if an equally perfect burning could have been secured without; because the water, as steam, has about twice the capacity for heat, as the other gases.

If we only add enough water to make all the carbon into carbonic oxide, it will take only half as much water; 125 pounds to 100 pounds of naphtha making about one-thirteenth of the gas after burning; and reducing the temperature only one-seventh.

But if we use only 36 pounds of water to 100 pounds of naphtha, one gallon of water to four gallons of naphtha, the gases may be something like this:

Marsh gas, CH_4, 80 pounds.

Carbonic oxide, CO, 56 pounds.

which with 1530 pounds of air (20,000 cubic feet) to burn it, will make 1666 pounds of gas; and the water added will only reduce the temperature one-twenty-fifth part, instead of so much as before.

In each case the air to burn it, and the units of heat produced, will be the same; but the temperature will vary with the proportion of water added, and, also, with the quantity of unburned air passing through.

One of the great advantages of the water process is the condition of blast with which the fuel unites with the air, by which the thorough mixture and burning is secured. And it is very important that the quantity of air going into the furnace be regulated so as to furnish only about

the right amount. All that is more or less than the exact amount required, reduces the temperature so much.

In ordinary furnaces, it has been estimated that much more than half the heat is lost by this one item alone. If the air passes freely in above the coal, twice as much goes in as is burned; if it all passes in under the grate, then only one-third the heat is given off, as only carbonic oxide escapes.

Probably the advantages with crude petroleum or with coal, of the water process, would be of still greater value.*

SAMUEL J. WALLACE.

WASHINGTON, D. C.

BOOKS RECEIVED.

OBSERVATIONS OF DOUBLE STARS made at the United States Naval Observatory by ASAPH HALL, Professor of Mathematics, U. S. N. Rear Admiral Rogers, U. S. N., Superintendent, Washington, 1881.

In introducing this work Professor Hall gives some very interesting details respecting the methods used in making observations at the Naval Observatory and the condition of the instruments.

He also presents his reasons for undertaking these observations and indicates the scope of the present work.

He states that his regular observations with the 26-inch refractor of the Naval Observatory were begun in the spring of 1875, the instrument at that time being in charge of Professor Simon Newcomb. "Professor Newcomb gradually withdrew from observing with this instrument, which came under my direction sometime in July of the same year. The micrometrical measurements which had been made by Professors Newcomb and Holden were chiefly of the satellites of Uranus and Neptune, and the discussion of these measurements of the two outer satellites of Uranus brought out very clearly what had been indicated before by Von Asten; viz, the existence of a large constant difference in the angles of position measured by Mr. Otto Struve, director of the Imperial Observatory at Pulkowa. As it is our intention to repeat the measurements of the satellites of Uranus and Neptune after a few years, and as it seemed probable that similar differences might exist in the observations of double stars, it occurred to me that the best way of comparing and uniting the observations of different astronomers would be for each one to observe the same double stars at nearly the same time. I wrote to Struve proposing that this should be done, and that he should select the list of stars. In reply he informed me that such a series of observations was already in progress between himself and Baron Dembowski, and after adding to the list of stars a few of greater distances, this list and an account of the proposed work were published by Struve in the "Vierteljahrsschrift der Astronomischen Gesellschaft." Band xi, p. 227.†

It was understood that each observer should avoid all knowledge of the observations of other astronomers, in order that his work might be done independently, and in my own case this rule has been carefully adhered to. But now nearly four years have elapsed since Struve's publication, and it is probable that all the astronomers engaged in this work have collected such a number of observations that the publ cation of my own results will not influence the independence of theirs. Moreover, the end of the year 1879 seems to be a favorable epoch for publishing my observations of double stars made before 1880, since I hope to make some changes which in the future will enable me to observe under conditions more favorable to accuracy.

I have therefore collected and revised all my observations of double stars, and the results are given in the following pages. In order to make this collection complete I have concluded the few observations made in the year 1863 with the equatorial of 9.6 inches aperture. The whole number of observations is 1614.

It will not be necessary to give any general description of the 26-inch refractor made by Alvan Clark and Sons for the Naval Observatory, since such descriptions can be found in the annual volumes of the Observatory for 1873 and 1874. It will be sufficient to say that the form of the mounting adopted by the makers for this Equatorial is such that the instrument, notwithstanding its great size, is handled with ease; and the harp-shaped piece that supports the polar axis is very convenient when observing near the zenith. Generally the instrument is pointed on a star by means of what are called the "rough circles." These circles are the edges of the hour and declination circles, which were painted white, and then divided by lines of black paint, the hour circle into spaces of ten minutes of time and the declination circle into degrees. This method of pointing is usually accurate enough to find the object, but as the painting was not well done errors as great as 15′ to 20′ could be made in some parts of the rough declination circle. An accurate reading for the position could be made by means of the finely divided circles, but this involves considerable time and trouble. On account of the delay in the observations which would be caused in making the change, and of the natural inertia in getting rid of a poor thing to which one has become accustomed, this defective circle for the declination was used until June, 1879, when the circle was painted white and divided again under the care of Mr. Gardner, the instrument maker of the Observatory. The settings are now much more accurate and give but little trouble, and the saving of time is very great. It is possible that a few cases may be found where, on account of an erroneous setting in declination, I have observed a different object from the one supposed.

The ease and rapidity with which observations can be made with a filar micrometer depend largely on the performance of the driving-clock. The accuracy of the observations also is in a measure dependent on this performance, but patience and skill on the part of the observer will in a good degree make up for a poor performance of the clock. The motive power of our driving-clock comes from a small water-wheel which is driven by water drawn from the Potomac water pipes. At first the water was applied directly to the conical pendulum, but the pressure of the water was so variable that weights attached to an endless cord (Huygen's loop), were placed between the water-wheel and the pendulum by Professor Newcomb. When this had been done the performance of the clock is said to have been tolerable; but in the autumn of 1875 it became very troublesome, and the observer was frequently annoyed by the stopping of the clock. This trouble continued and became worse until July, 1876, when the clock was dismounted by Mr. Gardner and myself. The lower end of the shaft of the conical pendulum had been given a conical shape, and had rested in a conical cup. The friction and heat had been so great that the lower end of this shaft had become very rough and twisted to a gimlet shape, thus stopping the clock. The bearing of the shaft was changed and made of a plane agate surface, the lower end of the shaft being rounded to a slightly curved surface. The friction of the upright shaft of the water-wheel was also diminished by clamping a set of friction wheels to this shaft and letting them play on a horizontal iron surface. The weights on the Huygen's loop were changed for cups carrying shot. With an average pressure of the water, and the machinery well oiled, these

* This superiority of the non-luminous combustion for heating was discovered by Professor Henry. He says: "With this arrangement the light of the flame was increased, while the time of bringing the water to the boiling point was also commensurably increased, thus conclusively showing that the increase of light was at the expense of the diminution of the temperature."

† Mittheilung über unternommene Beobachtungsreihen zur Vergleichung von Mikrometer messungen. 1876, Anfang Juni. OTTO STRUVE.

weights are $7\frac{1}{2}$ and $3\frac{1}{2}$ pounds, but the weights can be varied to suit the resistance and the pressure by changing the shot. Since these changes the performance of the clock has been tolerably good. Still this clock needs much care, and being dependent on an unsteady pressure of water a delay in the observations sometimes occurs. The great length of the telescope, which exposes it to the action of the wind, is also a hindrance to the steady driving of the clock.

The difficulty in turning the dome, of about 42 feet diameter, has increased. This difficulty is caused probably by the uneven settling of the supporting walls, and the bulging of the dome in the direction of the slit. The labor of turning the dome through a revolution is so great that lists of north and south objects are prepared beforehand by the observer in order to avoid as much as possible the turning of the dome.

After some practice, and on becoming familiar with the instrument and micrometer, my manner of observing a double star has been as follows: In order to measure the angle of position the two wires are separated a convenient distance and the stars are placed between them. The position-circle is turned by the hand until both stars appear midway between the wires, and then the circle is read. The light having been taken out of the micrometer, the wires are turned thirty or forty degrees forward and backward several times before the light is thrown on the wires again for the purpose of making the settings of the circle as independent as possible, and another reading is made. Generally four readings of the position-circle are taken. Then this circle is turned 90° from the mean of the readings and the double distance is measured. First the stars are bisected by the wires and the micrometer is read; then the wires are reversed and the stars are bisected again. The wires are then restored to their original position and another double distance is measured. Two such distances are generally observed. An estimated value of the angle of position is always recorded, as well as the sidereal time of the observation, and also an estimate of the weight of the observation. This weight depends simply on the condition of the images of the stars, and the numbers **1** to **5** are used for expressing the weights; **1** denoting a very poor condition of the images, **3** an average condition, and **5** a perfect condition. I have very rarely observed double stars when the images were so poor as to be given the weight **1**. As far as possible I have avoided all knowledge of the angles and distances observed by other astronomers. In my observing-list these quantities are omitted, and no comparison with other observations is made until my own observations of a star are completed. It is possible, therefore, that in some cases my angles may differ by a multiple of a quadrant from those observed elsewhere.

I have omitted observations of color and of magnitude. These observations have now become a specialty, and such observations as I could make would not do much more perhaps than tend to introduce confusion. In the case of stars observed by the Struves, to which most of my observations belong, I have adopted their magnitudes. In most cases these magnitudes are brighter than those of the scale to which I have been accustomed; thus what the Struves would call a 7th or 8th magnitude I would call an 8th or a 9th.

Very few of the observations have been made in the twilight, which offers the best conditions for observing double stars, since, the observer residing at a distance from the observatory, it has not been convenient to do this.

With such a large objective great changes occur in the appearance of the stars during a single night. Generally so long as rapid changes of temperature are going on the performance of the object-glass is not good. But on a few nights of the year, when all the atmospheric conditions are favorable, the performance of the glass is excellent, and its separating power is all that could be desired. Usually ruddy and reddish stars are the most difficult to observe, a result which may be caused by the figure of the objective. After having been in use two years the form of the lenses seemed to have undergone a slight change, and in the beginning of May, 1876, the surfaces of the flint lens were refigured by Mr. Alvan Clark and his son, Mr. Alvan G. Clark. This is the only change that has been made in the objective. On a single occasion water collected between the lenses, and they were taken out, cleaned by Mr. Gardner, and returned to their cell with very little trouble.

Until March, 1878, all the observations were made with my left eye; but having used my eyes very much during the preceding year, and having done a good deal of computing by gaslight, my eyes became weakened. In March, 1878, while observing the stars in the Trapezium of Orion with a field illumination which was very unsteady, my left eye suddenly became bloodshot. After a rest of a week the eye resumed its natural appearance, but on observing again the blood reappeared in the eye. I then began to use my right eye, and have used it since in most of the observations. From a number of trials I think that this change of eyes has produced only a small change in my habit of observing an angle of position. Still it is possible that some systematic difference in the angles may exist on account of this change, as there was at first some awkwardness in observing with my right eye. In all my observations the head of the observer was kept in an upright or natural position."

The elaborate introduction of Professor Hall leaves us little scope for further explanation. We may state, however, that the tables in which these observations are condensed cover nearly 150 folio pages, and will be accepted as a valuable addition to the literature of this subject, which has been much developed of late by the researches of Mr. Burnham and others.

A PARASITE IN ÆGERIA SYRINGÆ. HARR.

By G. H. French, Carbondale, Ill.

When examining the stems of some lilac bushes in my yard, I found a place in the bark of one where it seemed that an Ægerian pupa might soon protrude for the purpose of liberating the moth. Upon cutting away the thin film of bark, I found the end of a chrysalis visible. I carefully cut away the wood, took this out and put it in a jelly dish surrounded with lilac leaves to prevent its drying up, and waited for the imago to come forth. June 5th, a week after the chrysalis had been put into the jelly dish, I saw something among the leaves which I supposed was the expected moth, but which proved to be a hymenopter. I did not know but the insect might be one of the boring bees that often resort to the holes left by Ægerians in which to rear their young, but an examination of both the insect and the empty pupa case assured me that I had a parasite. The chrysalis was certainly that of an Ægerian, having all the characteristic marks of the pupæ of that family; and the insect in emerging from it had gnawed a hole near the end on the left side instead of the usual method of emergence of insects from their own pupa cases. More than this, the specimen was a true Ichneumonide and not a Crabronide as I at first thought it might be. This is the first time I have known of any parasite working in the Ægerians.

I make the parasite to be *Phæogenes Ater*, Cres. It is shining jet black, 40 of an inch long, the antennæ 25 jointed, the first 8 black, the next 4 white and the rest dark brown. The joints of the legs are a little pale.

It is impossible for me to say when the parasite was introduced into its host; but it must have been before it pupated, because the chrysalis when taken from the bush was entire, showing no broken place. That the Ægerian was *Æ. Syringæ*, I have no doubt, I do not know of any other boring in the lilac.—*Papilio.*

TABLES FOR QUALITATIVE CHEMICAL ANALYSIS, with an Introductory Chapter on the Course of Analysis, by Professor HEINRICH WILL of Giessen, Germany. Third American, from the eleventh German, edition. Edited by CHARLES F. HIMES, Ph. D. Henry Carey Baird & Co., Philadelphia, 1881. Price, $1.50.

In this work a series of fourteen tables are presented which will be found of the highest value to the chemical student, and will be the means of saving a large amount of time if used by those engaged in chemical analysis. These tables are compact, but sufficiently explicit, and the summary view of the general course of qualitative analysis, and of the classification of compounds, according to the properties relied upon for their detection, afford a thread, as it were, around which chemical facts may crystalize as they accumulate. These tables appear well adapted for a course of college studies, and their popularity and scientific character is indicated by their general adoption in the German Universities. With such an endorsement, we anticipate a large sale for this book among American students of Chemistry.

TRICHINÆ IN RATS.

In regard to Dr. GLAZIER'S belief that rats are not the "headquarters" of trichinæ elaborate, expressed in his official report on trichinæ and trichinosis, the following, taken from the *Geitschrift für unkros Kopische Fleischschan*, is of interest:

Dr. MERKEL, County Physician at *Nuremberg, Bavaria*, had asked the Microscopical Society at that city to examine as many rats for trichinæ as they could collect for the purpose. He distributed blanks among the members, which he requested to be filled. Within six months 111 of these troublesome animals had been so examined, with the following result:

Of 40 rats caught at or near abattoirs, 8 (20%) showed trichinæ, while 71, caught on private property, showed 8 (11.27%); total, 111 rats, showed 16 (14.4%).

This would certainly confirm the idea that the neighborhood of those places where swine will devour anything that offers—which they would presumptively do otherwise only after having been fed—rats are more dangerously infected than where the porcine tribe is more regularly cared for.

SOME NEW FACTS ABOUT RABIES.—It is known that M. Pasteur is directing his attention to the subject of rabies. The virus of that disorder of course exists in the saliva, but M. Pasteur has now proved that it does not exist there only. The brain substance also contains it, and, used to inoculate healthy animals, will reproduce the disease as effectively as the saliva. Matter from the medulla oblongata and the frontal portion of one of the brain hemispheres and the liquid of the brain have been thus employed with success. Again, one of the great difficulties in research on rabies arises from the uncertainty of development of the evil after inoculation or a bite, and the long time of incubation. M. Pasteur is now able to communicate the disease surely, and to shorten considerably the time of incubation. His method is to inoculate directly the surface of the brain, having recourse to trepanation, and using as inoculating matter the cerebral substance of a mad dog as pure as possible. In that case the first symptoms of rabies appear infallibly in a week or two, and death ensues in less than three weeks. In these researches, of which we may expect to hear more shortly, M. Pasteur has the co-operation of MM. Chamberlain, Roux and Thuilier.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING JULY 30, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

JULY.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.
Sunday, 24	29.812	29.862	12 p. m.	29.722	0 a. m.
Monday, 25	29.809	29.862	0 a. m.	29.744	6 p. m.
Tuesday, 26	29.730	29.748	0 a. m.	29.704	5 p. m.
Wednesday, 27	29.726	29.776	9 a. m.	29.698	6 p. m.
Thursday, 28	29.751	29.836	12 p. m.	29.702	0 a. m.
Friday, 29	30.017	30.102	12 p. m.	29.836	0 a. m.
Saturday, 30	30.164	30.190	1 p. m.	30.102	0 a. m.

JULY.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Maximum. Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Minimum. Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 24	76.3	68.3	85	3 p. m.	73	3 p. m.	65	5 a. m.	63	5 a. m.	143.
Monday, 25	77.6	72.0	86	4 p. m.	76	4 p. m.	71	5 a. m.	67	5 a. m.	138.
Tuesday, 26	76.3	70.3	81	3 p. m.	72	10 a. m.	71	12 p. m.	69	12 p. m.	130.
Wednesday, 27	71.3	67.0	80	5 p. m.	70	6 p. m.	65	5 a. m.	65	5 a. m.	127.
Thursday, 28	71.3	67.0	78	1 p. m.	70	5 p. m.	63	5 a. m.	63	5 a. m.	143.
Friday, 29	68.0	67.6	72	3 p. m.	70	3 p. m.	64	5 a. m.	64	1 a. m.	126.
Saturday, 30	64.6	64.0	68	11 a. m.	66	3 a. m.	64	12 p. m.	64	12 p. m.	94.

Mean for the week .... 29.858 inches.
Maximum for the week at 1 pm., July 30th .... 30.190 "
Minimum " at 6 pm., " 27th .... 29.698 "
Range .... .492 "

	Dry.		Wet.	
Mean for the week	72.2	degrees	68.0	degrees.
Maximum for the week, at 4 pm. 25th	86.	"	at 4 pm 25th, 76.	"
Minimum " " 5 am. 28th	63.	"	at 5 am 28th, 63.	"
Range " "	23.	"	13.	"

JULY.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.
Sunday, 24	n. n. w.	s. w.	s.	77	1½	5.50 pm
Monday, 25	s. w.	s.	s.	187	4	6.15 pm
Tuesday, 26	s. s. w.	w. n. w.	w. s. w.	203	4	0.20 am
Wednesday, 27	n. n. w.	n. w.	s.	88	1	4.00 pm
Thursday, 28	n. w.	w. n. w.	s. s. e.	102	1¼	3.00 pm
Friday, 29	n. e.	e.	e.	123	4¼	0.50 pm
Saturday, 30	n. e.	n. e.	n. e.	199	5¾	12.00 m

JULY.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.
Sunday, 24	.537	.597	.628	71	53	72	0	2 cir. cu.	0
Monday, 25	.641	.746	.744	76	64	86	2 cir.	7 cir. cu.	2 cir. cu.
Tuesday, 26	.731	.585	.666	90	55	77	9 cu.	7 cu. s.	5 cu.
Wednesday, 27	.626	.591	.595	94	68	76	9 cu.	7 cir. cu.	0
Thursday, 28	.536	.614	.668	84	68	85	1 cir.	9 cu.	0
Friday, 29	.617	.745	.662	100	95	100	10	8 cir. cu.	5 cu.
Saturday, 30	.583	.583	.596	94	94	100	10	9 cu.	10

JULY.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Begining.	Time of Ending.	Duration. h. m.	Amount of water	OZONE.
Sunday, 24	-----	-----	-----	--	0
Monday, 25	-----	-----	-----	--	0
Tuesday, 26	-----	-----	-----	--	1
Wednesday, 27	-----	-----	-----	--	2
Thursday, 28	-----	-----	-----	--	0
Friday, 29	-----	-----	-----	--	9
Saturday, 30	0.30 pm 8.00 pm	2 pm. 12 pm.	1.30 4.00	.05 .15	6

Distance traveled during the week .... 979 miles.
Maximum force .... 5¾ lbs.

Total amount of water for the week .... .20 inch.
Duration of rain .... 5 hours 30 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.